addressed to Dr. J. H. Gilbert, and dated "Down, February 16, 1876." The following passage is the one which interests us here:—

"It is admitted by all naturalists that no problem is so perplexing as what causes every cultivated plant to vary, and no experiments as yet tried have thrown any light on the subject. Now for the last ten years I have been experimenting in crossing and self-fertilising plants; and one indirect result has surprised me much, namely, that by taking pains to cultivate plants in pots under glass during several successive generations, under nearly similar conditions, and by self-fertilising them in each generation the colour of the flower often changes, and, what is very remarkable, they became in some of the most variable species, such as Mimulus, Carnation, etc., quite constant, like those of a wild species."

We now know that the colour changes and the becoming constant to which Darwin refers were the results of the repeated self-fertilisation of heterozygous material, so that the supposed variability evidently was nothing but segregation after a cross.

Velp, Holland, November 2. J. P. Lotsy.

It would take too much space to reply in detail to all of Dr. Lotsy's statements, for which I have great respect. They go far outside the original point at issue, but it is necessary to refer to the more important of them, and it will then probably be seen that the others are immaterial. In his original letter (Nature, October 27, p. 274), which commented on an article of mine on "British Roses and Hybridity" (Nature, September 15, p. 99), he states that Jeffrey's work tends "to show that the presence of 'bad pollen' is *proof* [my italics] of a hybrid origin," and goes on to say that this view is "much strengthened" by other work. He correctly states that I took exception to that view, my own view being that "bad pollen" is unsafe as a criterion of hybridity, in support of which I cited various facts. As some of these facts were from a paper of which I was joint author, the original article was unsigned, but since this controversy, which was not of my seeking, has arisen, I prefer to sign my own name. In his present letter Dr. Lotsy seems to forget that the burden of proof rests upon those who assume that bad pollen is a proof of hybridity. He says that my postscript to his article is "not according to facts," and that he did not suggest the hybrid nature of Trillium, Dirca, and Scoliopus. I can only ask, if that is the case, why did he refer to them in his original article? Cytological work, which is by no means all "recent," proves that hybridity is *a* cause of bad pollen, but by no means proves that it is the only cause.

Dr. Lotsy has apparently omitted a consideration of lethal factors from his views. This is a more recent discovery which is of much significance in the interpretation of sterility, not only in Drosophila, but in various Œnothera forms, and it may apply either to gametes or zygotes. The conception has already been fruitfully applied, not only to various plants and animals, but also to man himself. When I said that the theory of mutation requires the occurrence of a certain proportion of defective germ-cells I had similar cases in mind, and did not mean to imply that mutations were necessarily *always* accompanied by germ-cell sterility. But clearly, if lethal factors account for germ-cell sterility in some cases, it is inadmissible to assume that bad pollen is in itself a proof of hybridity. We must apply the conception of multiple causes.

It follows that in any given case, such as that of *Oenothera Lamarckiana*, bad pollen *may* have originated from crossing, from lethal factors, or from some other cause, unless one or more of these possible causes can be eliminated. Dr. Lotsy says, "We now know that *O. Lamarckiana* is a hybrid." One can only ask how we know, and in what sense he is using the term hybrid. So far as the theory of mutation in Œnothera is concerned, it no longer matters whether *O. Lamarckiana* is a garden hybrid or not, since the work of Bartlett has proved that various close-pollinated wild American species of Œnothera show the same mutation behaviour.

Finally, I would say that if by his theory of evolution by hybridisation Dr. Lotsy means merely that the intercrossing of related races is the condition in which evolution has frequently taken place, I, for one, would heartily agree with him. For I have long advocated the view that among open-pollinated plants and most animals the evolutionary unit is an interbreeding population of closely related forms. I fancy many biologists adhere to a similar point of view. But I take it that Dr. Lotsy means much more than that by his theory, and if I understand him correctly, that is his reason for tacitly denying the existence of germinal changes. One can only ask how two visibly similar homozygous organisms when crossed can give rise to new germinal characters if they have not during the previous period of their isolation undergone germinal changes. R. Ruggles Gates.

King's College, Strand, November 11.

Biological Terminology.

(*a*) "Variation is the sole cause of non-inheritance"; (*b*) "Apart from variations like exactly begets like, when parent and child develop under like conditions"; (*c*) "The development of the individual is a recapitulation (with additions and subtractions due to variations) of the evolution of the race." Here are three statements which seem to me "*in effect*" identical. To Dr. Bather the first two seem identical, but not the third. But if the child in his own development step by step recapitulates (with variations) the development of the parent, and the parent in the same way recapitulated that of the grandparent, and so on to the beginning, how, in the world, can the development of the individual be anything other than a recapitulation (with the accumulated variations) of the evolution of the race? If that be so, does not (*b*) necessarily involve (*c*)? (*c*) is merely (*b*) applied to a succession of parents and children. Dr. Bather says (Nature, October 27, p. 271) that this is not what biologists mean. Then what do they mean? "Recapitulation" must be one of those terrible words which, like "inherit," are used, quite unconsciously, with a number of diverse and even contradictory meanings.

It is pleasant to find that Dr. Bather approves of Prof. Goodrich's address, for probably it has set the heather alight at last. In my humble way I also am enormously pleased. Still, Dr. Bather should bear in mind the history of this matter, some of which Prof. Goodrich indicates. As long ago as the 'eighties Weismann declared that "an organism cannot acquire anything unless it already possesses the predisposition to acquire it." At that time, too, doctors were beginning to insist that not actual diseases, but only predispositions to acquire them, were inheritable. Weismann failed to perceive the necessary consequences of his own idea—predisposition is all that can be inherited in the case of any character; all characters, therefore, are equally and in exactly the same sense innate, acquired, and inheritable. Instead he assumed, with Lamarck, that some characters are innate and others acquired, and so started the famous—or infamous—Lamarckian controversy. Sandeman did, however, very definitely

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follow out the idea in 1896, as did Adam Sedgwick in 1899. If authority be necessary, here is authority in plenty. I also tried in my very humble way, beginning as long ago as 1906. I daresay each man in turn thought he was propounding something new. Dr. Cunningham perceives, I hope, that Prof. Goodrich is spared the disgrace of being my pupil, and that even the most self-respecting biologist may, in this instance, follow the truth without qualms of conscience. Dr. Bather knows with what reception I met. I was told that I was doing harm, that biologists could manage their affairs quite well without my help, and so forth. Then the worm turned. So far as I am able to judge, Dr. Bather objects to my letters because they are tediously long and because they are impudent. Certainly they are long, and doubtless they are tedious. But I could state, or assume, in half a dozen words a fallacy which Dr. Bather could not refute in less than half a dozen columns. Moreover, as Dr. Bather courteously indicates, it has been holiday time, during which one does unusual things; therefore I have used his letters —with all reverence, as a parson might—as texts whereon to hang admonitory discourses. Certainly these letters have been impudent—most impudent. But here, again, we have the trodden worm.

Dr. Bather thinks I ought not to discuss variations unless I first account for them, which is like saying I ought not to eat my dinner unless I first cook it. Must I not accept the given fact? I am at once accused of being tediously long and not long enough. What is a poor man to do? Besides, I have tried elsewhere ("The Laws of Heredity," chap. 5) to do this very thing. Primarily variations can arise only in two ways. Either they are impressed on the germ-plasm by its environment, or they occur because the germ-plasm is a living, growing, changing thing which, like other living things, tends to revert to the normal from impressed change, especially injury. There exists ample crucial evidence to enable us to reach a decision, but much of it lies, outside the high roads followed by biologists, in the realms of disease and bacteriology.

Dr. Cunningham's letter (NATURE, November 17, p. 368) is addressed especially to Prof. Goodrich, who may deal with it if he desires; but one passage refers to my particular hobby. Lamarck called the changes which result from use "acquired"; but, thinking only of trifling changes which occur at the end of the development, he did not realise that the growth of the higher animals, especially man, is due mainly to that functional activity which begins to act immediately after birth. His successors employed the word "acquired" as indicating any character which develops under any very glaring influence. Now Dr. Cunningham defines an acquired character as a "change" (from the person's antecedent self, from the parent, from the race—which?) due to "environment or modification." In that case the English language is not "acquired," but is "innate" in an Englishman. If learned by a Frenchman, it is acquired. Heaven knows what it is if learned in Jersey. He accuses Prof. Goodrich and me of a "misuse of words" and of obscuring "a *perfectly clear* distinction"! The italics are mine!

G. ARCHDALL REID.

9 Victoria Road, Southsea, November 19.

The Softening of Secondary X-rays.

DR. A. H. COMPTON in a letter on this subject in NATURE of November 17, p. 366, described an experiment in which he reflected the $K\alpha$ rays from a molybdenum Coolidge tube on to a slab of paraffin, and measured the absorption coefficient of the secondary scattered rays at different angles with respect to the direction of the primary beam. The absorption coefficient of the secondary rays was found to be 29 per cent. greater than that of the primary $K\alpha$ beam at $\theta=90°$, and 6 per cent. greater at $\theta=20°$. This softening of the rays on being scattered was still more pronounced when the K lines of tungsten were used.

Dr. Compton referred to this work as a repetition of measurements which I had previously reported (*Phil. Mag.*, September, 1921), in which no such increase in absorption after scattering was observed, and he attributed my negative result to an unfavourable choice of wave-length and angle. Apparently he did not understand the purpose of my experiment. It was to settle a question regarding the interpretation of energy measurements made with the Bragg spectrometer. We were not sure that the atom in a scattering substance does not always absorb energy from the incident rays and re-emit this energy in a manner characteristic of the atom and independent of θ. My problem was to find out if such an effect need be considered in ordinary spectrometer measurements. The wave theory of scattering predicts a certain amount of softening due to the finite size of the atom and to a sort of Doppler effect, but not nearly the observed amount, especially at large angles.

As Dr. Compton suggests, there is probably an additional somewhat softer radiation due to collisions of electrons released within the scattering substance by the primary rays. Such a "fluorescent" radiation should diminish with θ, as observed. The softening due to the finite size of the atom should also, in general, diminish with θ and be negligible in the characteristic radiation, which is believed to consist of relatively sustained wave-trains. Softening due to these recognised causes can thus be minimised by using the sustained characteristic rays, large wavelengths, and θ as small as possible. I chose these conditions, which were unfavourable to the Compton effect, because I wanted to eliminate it so far as possible. The negative result simply indicates that with light atoms the indirect unpolarised radiation sought is not great enough to require consideration in ordinary crystal measurements. S. J. PLIMPTON.

Worcester Polytechnic Institute, Worcester,
Massachusetts, November 8.

The Molecular Scattering of Light in Liquids and Solids.

As was pointed out by the late Lord Rayleigh, the basis of his theory of the blue sky, namely, that the molecules scatter the incident energy independently of each other's presence, is only true for gases in consequence of the freedom of movement the molecules possess in this state of matter. In connection with the problem of the colour of the sea and of deep waters generally it is necessary to know the scattering power of ordinary liquids, such as water, and I find this can be very simply accomplished by application of the theory of local fluctuations of density arising from molecular movement, originated by Einstein and Smoluchowski and utilised by the latter to elucidate the phenomena occurring near the critical state. The general formula for the scattering power of a fluid is

$$\frac{\pi^2}{18}\frac{RT\beta}{N\lambda^4}\left(\mu^2-1\right)^2\left(\mu^2+2\right)^2,$$

where β is the compressibility of the substance, μ its refractive index, R, T, N being the usual constants of the kinetic theory. The scattering power of water comes out from this formula as about 160 times that

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